PRESENTATION OF THE MARY KINGSLEY MEDAL

TO

DR. GRIFFITH EVANS

The North Wales Branch of the British Medical Association on 14th December, 1917, gave a luncheon at the Imperial Hotel, Colwyn Bay, in honour of Griffith Evans, M.D., C.M. (McGill), M.R.C.V.S., of Brynkinallt, Bangor, on the occasion of a presentation to him of the 'Mary Kingsley Medal for distinguished Scientists who have assisted the cause of Tropical Medicine by original research.'

The Committee of the Liverpool School of Tropical Medicine had unanimously agreed to offer to Dr. Evans the Medal of the School, which was struck in commemoration of the work of the late Miss Mary Kingsley in West Africa.

At the luncheon Dr. H. Drinkwater, of Wrexham, presided, and in proposing his health said that their distinguished guest was the pioneer in the study of Protozoology in connection with infections, and had added something of permanent value to the sum of human knowledge.

Professor J. W. W. Stephens, of the Liverpool School of Tropical Medicine, formally presented the Medal of the School to Dr. Evans, and also the following address:—

> 'We, the undersigned members of the Professional 'Committee of the Liverpool School of Tropical Medicine, 'desire to offer you our hearty congratulations on the presenta- 'tion of the Mary Kingsley Medal to you in recognition of your 'distinguished scientific work. We recall that you were the 'first to associate trypanosomes with the production of disease, 'and the specific name of the trypanosome of surra which you 'discovered will always perpetuate your name in connection 'with that discovery. All the more honour is due to you also 'for maintaining the correctness of your view, that the trypano- 'somes caused the disease surra, in the face of official opinion to 'the contrary. We trust that in due time we may have the 'pleasure of honouring you in Liverpool.—We are, yours 'respectfully, Richard Caton, Chairman; J. W. W. Stephens, 'Professor of Tropical Medicine; Robert Newstead, Professor of

'Entomology; Warrington Yorke, Professor of Parasitology, 'B. Blacklock.'

Dr. Griffith Evans, in reply, said he felt highly gratified by the honour which had been conferred on him.

He proposed the toast of 'The Liverpool School of Tropical Medicine,' and said that the School had done incalculable work in the cause of the scientific pathology of the tropics.

Professor Stephens, in responding, said that the School was still hard at work, but the new laboratories into which they had proposed to move were at present being used as a military hospital. He hoped soon to be able to welcome Dr. Evans and the members of the Association there.

AUTOBIOGRAPHICAL MEMOIR

I was born at Tymawr, near Towyn, Merioneth, 7th August, 1835, being the only son of my parents, Evan and Mary Evans.

Educated at the British School, and by private tuition.

Was for a short time pupil of John Pugh, F.R.C.S., Aberdovey and Towyn, but circumstances diverted me to the R.V.C., London, to qualify for appointment as Veterinary Surgeon in the Army, and I obtained a commission in the Royal Artillery, January, 1860. That was in the Regimental time, before the Departmental system was thought of, and eleven years before the purchasing of combatant commissions was abolished. I mention this to show what social changes I experienced, and to say I have very happy recollections of the old régime.

I went to Canada with troops in the famous S.S. 'Great Eastern,' because of the U.S. Civil War, and was stationed at Montreal, June, 1861. Registered in the Medical Faculty, McGill University, without delay, and graduated M.D., C.M., in 1864. The subject of my graduation thesis was 'Tuberculosis,' giving evidence of its infection, character, and advocating the open-air treatment. Professor Fraser, who had read my thesis, challenged the infection in Convocation, but I maintained my ground by added evidence of my own observation. I continued working there for another year, especially in regional anatomy, surgery, and clinics.

In 1865 I spent two months visiting the Field Hospital of the Northern Army (there I learnt how much better the medical and surgical cases did in tents than in the best walled hospitals of the period), being the first English Officer allowed to go after the great disaster in 'The Wilderness,' etc.

Afterwards I was stationed at Toronto, when I became acquainted with a charming young man, then beginning his medical study as pupil of one of the leading general practitioners there, now the Regius Professor of Medicine, Oxford—Sir William Osler.

I returned with troops to England, July, 1870, in the Indian Troopship 'The Crocodile,' specially sent on account of Prince Arthur, then a subaltern in the Rifle Brigade, who was returning with his battalion. The Queen's yacht, 'The Victoria and Albert,' came to meet his arrival in the Channel to take him to Osborne, and brought news of war declared by France against Prussia, which was a great surprise to us all.

I went with my battery to Ipswich, where I was stationed for nearly a year, and was most kindly privileged by the Medical Staff of the Infirmary to benefit by their practice. I was there almost every day.

One day a young son of a sergeant in my battery was knocked down in the street by a dog-cart, the wheel of which passed over his head, wounding him severely. He was carried to the Infirmary unconscious. His mother came to me that evening in her distress, begging me to treat him if she brought him home. I persuaded her to leave the boy where he was, promising to see him every day. When I arrived there one day I was told he had tetanus, and there was going to be a meeting to consult *re* treatment, to which I was invited. There had been a remarkable run of tetanus, I forget how many cases, from the mechanical works mostly, and all had proved fatal, though every recognised method of treatment had been adopted. Amputation had been tried in a case of injury to the end of a finger. I was asked to give my opinion. I replied that I regarded the disease as a specific fever, due to some specific cause, and for which there was no known specific remedy. It had to run its course like other specific fevers, and our duty was to keep the patient in the best possible position for self-recovery, that was, to favour as much easy rest as possible, avoid everything that might excite the spasm, keep in a dark silent room, no noise from without or within, give no food of any kind, nor any medicine, but let the patient drink water *ad lib.* No one should go in to see him except a specially selected nurse and the House Surgeon. They all protested against not giving any medicine of any kind; if the patient died they would blame themselves for neglect. After some conversation, I was asked would I venture the risk if I were responsible for the treatment? I replied, I would without hesitation, with the consent of the parent. They all agreed for me to take charge of the case, the patient remaining in the Infirmary. The mother was called in, the subject was explained to her, the fatality of previous cases, etc., and she expressed her willingness to comply with any treatment I recommended, and promised also for her husband.

That case recovered. It is the only case I ever had to treat in man, but I have treated a number of cases in horses on the same principle, and all recovered.

In the autumn of 1870, I married Catherine, only child of John Jones, M.R.C.S., who had an extensive country practice at Llanfaircaerinion, Montgomeryshire. I was tempted to settle there with him, but was too fond of the roving military life.

In 1871, I exchanged from the Royal Artillery to the Army Service Corps, and was posted at Woolwich, with a prospect of remaining there several years. It suited my fondness to prosecute my medical studies, and I arranged to work through a post-graduate course of Practical Histology and Experimental Physiology at King's College, London; also a six months' course of lectures and clinics at the Royal Ophthalmic Hospital, Moorfields, where I was fortunate in having the friendship of Jonathan Hutchinson, Senior, who most kindly invited me to his clinics at the London Hospital, where I learnt much.

You ask 'especially' for 'some fuller details of my discovery of the *Trypanosoma evansi* than one finds in books.' Perhaps I had better tell you first how I became prepared to find it, and to try not to be too prolix.

Microscopy was my hobby since my earliest student days. I had kept myself informed of Pasteur's investigations and discoveries of pathogenic bacteria, and was deeply impressed with the conviction that a new door was opened for great developments in medical science. When I was sent to India in November, 1877, I provided myself with a portable microstand, with the best lenses I could obtain up to $\frac{1}{12}$th immersion, with suitable condenser, sub-stage, etc.

On arrival in India, I was sent to Sialkot in the Punjab, to investigate a disease that was endemic, and had been for many years extremely fatal to cavalry and artillery horses there, and at other stations in India. The symptoms varied extremely in each outbreak, according to the organs chiefly affected, respiratory or alimentary in different patients. I proved it to be anthrax fever, by finding the specific bacillus in the living circulating blood of every patient. I officially reported, what surprised me most, that the first change in the blood seen by the microscope was a great increase in the number of the large white corpuscles before I could see a bacillus. I examined the blood regularly every hour from the first symptom of illness, and noted invariably the increasing number of these corpuscles for some time before I could find a bacillus. The bacilli, when they came, appeared to be closer to the white than to the red corpuscles; subsequently the number of the bacilli in each droplet multiplied rapidly, so they could be seen isolated, free from corpuscles. I expressed my conviction that the large granular corpuscles had a very important relation to the bacilli, but I could not think what it was. I repeatedly emphasised my belief that it deserved special investigation. You will notice that was before Metchnikoff discovered them to be phagocytes, as published in 1884. I did not know, in 1878, how to fix and stain microbes in the blood for microscopic observation, so I floundered on, observing what I could.

In August, 1880, I was officially requested to proceed to Dera Ismael Khan, to investigate a disease known as 'Surra,' that had been very fatal to horses and camels of the Punjab Frontier Force for many years. I asked to be furnished with all the reports made upon it by surgeons, human and veterinary. After reading them, I was of opinion it was due to some parasite in the blood, though that was not suggested by anybody else. I expressed that opinion to the head of my Department, and stated that I could not undertake to investigate the disease unless I was fully authorised to kill as many of the patients as I wished in any stage of the disease for examination post mortem; and to make any experiment I might wish to transfer the disease to any healthy animal I would select for the purpose, so that I might know whether it was transferable, and, if so, that I might be able with certainty to study it from its earliest stage onward.

The progress of the disease was notoriously slow, and no one had been able to recognise it until after it had made some progress in wasting and general debility. Strong objections were made to giving the authorisation I requested. The question was ultimately referred to the Lieut.-Governor of the Punjab personally, and after some further cross-firing he decided entirely in my favour. Orders were issued to all concerned to give me every facility.

On arrival at Dera Ismael Khan, I called upon Dr. Haig, Surgeon of the cavalry regiment, and invited him to go with me to examine microscopically the blood I would take from a surra patient. (There was no European veterinary surgeon with the native cavalry.) Fortunately, after I examined the first droplet, I was able to say, 'Look at that, alive with microbes, such as I have never seen before: tell me what they are, if you know.' Dr. Haig did look, and was greatly astonished. He did not know what they were, but because of their great activity and their apparent onslaught upon the red corpuscles he suggested they should be named 'ferox.' That is how I discovered what have been given my name, firstly *Trichomonas evansi*, afterwards called the *Trypanosoma evansi*.

I selected two healthy horses which had not been near a case of surra. I poured the living blood of surra into the stomach of one, and under the skin of another. They sickened, and the special microbe in question swarmed in every droplet of their blood on the sixth day. The experiment was repeated with two other healthy horses, with the same effect. The four developed all the symptoms which were regarded as characteristic of surra.

I found that the blood with the microbes swarming in it when drawn, if set aside in an open or closed vessel to clot and cool, would become clear after twenty or twenty-four hours. I poured such blood into the stomach of a healthy horse, and injected it under the skin of another, with no effect in either case.

I communicated the disease from a horse to a dog and to a bitch, likewise by subcutaneous injection of the blood, and by the stomach. They sickened, and the microbe appeared in their blood. The bitch had a young pup that became affected with the disease by sucking her. I could not account for it otherwise, though I did not find the microbe in her milk. It was necessary to make further experiments carefully to decide that question, but I never had an opportunity.

I do not know whether anyone else experimented in that direction. I reported what I observed for others to continue.

I did not find an increase of white corpuscles in the blood *before* the appearance of the microbe in the cases communicated, as above stated, from horse to horse: but I noticed them in a few hours after, as recorded in my official reports thus:—'Beside the presence of the parasite, the elements of the blood became abnormal. The first apparent change is an increase in the number of white corpuscles; sometimes one-third or even half the number of corpuscles in the field will be white, and a remarkable increase in the proportion of the *large granular* white corpuscles.' But the dog and the bitch to which the disease was communicated as above stated had a great increase of the large granular white corpuscles before I found the microbe in the blood.

It was not known before I made that experiment that dogs were subject to surra disease.

I brought the infected pup with me on my return to Army Headquarters at Simla, in order to study the progress of the disease in him, and to learn what I could by passing the disease on from him to other animals, to know whether the disease with the specific microbe in question is really communicable by the milk only from mother to sucking pup, and whether the microbe may be found in the milk. Moreover, I was very anxious to show the living active microbe to other medical men, particularly to Dr. Cunningham, the Surgeon-General in India, and to Dr. Timothy Lewis, the Special Assistant to the Sanitary Commissioner with the Government of India, who had distinguished himself by his discoveries of blood parasites, officially reported and published in his illustrated monograph, a copy of which I have given to the Welsh National Library—it has been for a long time exceedingly scarce in the market.

Dr. Lewis, after examining the microbe in the blood of the pup, declared it was morphologically like what he had discovered in the blood of the brown rats of drains, described by him in the Journal of the Royal Microscopical Society, January, 1879, with some slight difference. These are now known as *Trypanosoma lewisi*. He emphatically controverted my opinion that they were pathogenic. The rats were, in his opinion, healthy.

In my official report upon surra in horses and camels at Dera Ismael Khan, I stated 'During the progress of the disease the parasite does not remain always in the same proportion, *it comes and goes in successive broods*. The general symptoms are also variable. I have not been able to prove that a definite relation exists between the variable number of the parasites present and the course of the symptoms, but I think it is probable.' Relation was afterwards proved to exist by Steel's observations upon surra in Burma.

The following extracts from my records of cases in the appendix to my report may interest you:—

CASE 1. 'It is worthy of note in this case that the urine became slightly albuminous with the disappearance of the parasites from the blood. I found nothing abnormal in the deposit from the urine examined carefully for casts.'

CASE 2. 'Note that in this case albumen was found in the urine throughout, and it appeared to have no relation with the disappearance of the parasite from the blood, as Case 1 led me to suppose. The structure of the kidney—post mortem—was perfectly normal under the microscope, and I found no casts in the urine. The structure of the liver was also normal, though all the mucous membranes were tinged yellow. I examined these organs with great care in this case, as in Case 1, directly after death (the animals were killed purposely for my examination). The weather was exceedingly hot, and the temperature within a cool bungalow about 82° every day, but I had plenty of ice in large felted baskets to preserve the organs while I examined them. . . I examined the dung carefully morning and evening for worms, etc., and found nothing abnormal. With regard to the swelling of the submaxillary gland and discharge of nasal mucus, I found this in four cases only out of fifty.'

CASE 3. 'It should be particularly noted in this case, with regard to the life-history of the blood parasites, that they do not disappear from the blood entirely when it becomes difficult to find one in a drop of blood; but that when one brood or generation dies, there are ova or spores left for the development of another brood. Thus, in this case, on the 26th September the parasites were swarming in each drop of blood. On the 29th, only one parasite was found in two drops of blood examined; on the 7th October great numbers were found in a drop—there were more than I could count on a slide. I was not able to see this case again from the 9th until the 16th, when no parasite could be found in a drop of blood examined.' (I had been away visiting other Posts on the frontier.)

CASE 5. 'This case has fully confirmed the experiments of Case 4 with regard to the disease being communicable by the subcutaneous injection of the blood containing the parasite, and it even more strikingly illustrates the short stage of incubation as compared with the first two experiments. The parasites were found swarming in the blood of this case, and the visible mucous membranes assuming the appearance peculiar to surra *four and a half days after the inoculation.* The next point of note is the shock the nervous system received a few hours after the inoculation, as shown by the intermittent pulse, the difficulty of feeling the pulse except at the heart by auscultation, and the lowering of the temperature. It is surprising how little the appetite of this case was affected afterwards, how rapidly the animal wasted while he ate as well as he did in health.'

'No local irritation followed when the hypodermic syringe punctured the skin in the operation of inoculation in any experiment.'

CASE 6. 'No parasite found in the blood of this mare five and a half days after she drank the surra blood, but two days later they were found swarming in each drop. The outward symptoms characteristic of surra appeared with the advent of the parasites. In this case, as in those inoculated under the skin, there was first a depression and then an abnormal elevation of temperature marking the transition period from health to disease.'

CASE 7. 'The mucous membranes first showed the peculiar yellow colour of the surra on the fifth morning after the surra blood was taken into the stomach, and the parasite was not found in the blood in this case until the seventh morning. This case fully confirms the experiment of Case 5, that the disease surra, with its peculiar blood parasite, may be communicated by drinking the blood of surra as well as by subcutaneous inoculation; also that it takes longer for the virus to pass into the circulation by drinking much of the blood than by injecting a little of it beneath the skin. The experiments prove that the common belief of the seeds of the disease lying dormant in the system for many weeks is entirely wrong.'

These extracts of the detailed reports I made of the cases show you the drift of my observations. You will appreciate the strong wish I had to find the meaning of the leucocytosis in relation to the parasites of surra and of anthrax fever, and why the surra microbe came and went so regularly in course of the disease—in the dog as in horses and camels. Dr. Lewis had not observed it in rats affected with a similar microbe, nor had he read of any other microbe in the blood of any animal conducting itself in that manner. He did not think that, nor the leucocytosis, of any importance worth troubling about.

Both Dr. Cunningham and Dr. Lewis had in their official reports committed themselves positively to the opinion that *no microbe found in the living blood of any animal was pathogenic*. They did not doubt my reports of what I had observed in anthrax and surra, but they, as strongly as they could use words officially, negatived the conclusions I drew from my observations. We discussed the subject in private conversations, of course in the most friendly spirit.

While you read the following extract of my report for the information of His Honour the Governor of the Punjab, dated November, 1880, bear in mind it was some time before Koch published his classical postulates. I was groping in the dark with psychological rushlights only, impelled by a very strong scientific faith that the discovery of important pathological facts was imminent in the direction I was trying to go.

The question suggested by these facts is, whether the presence of the parasite is the cause of the disease, or whether the disease is the cause of the appearance of the parasites. That they are most intimately related to each other is, in my opinion, beyond reasonable dispute. There are some eminent pathologists in India who deny the parasite origin of specific blood diseases; they say the cause of all such diseases, from smallpox to anthrax fever, is not any organic spore, or germ, or parasite of any kind, but it is some purely chemical agent which has never been discovered, and these organisms develop at once in blood which has been so chemically altered, each chemical virus developing its specific organism; the spores, or ova, of which are *supposed* to be in normal blood, ready to develop as soon as chemistry favours them. The organisms themselves, when developed, are supposed, by these authorities, to be harmless.

'That is one of the most vexed questions of the present day in the pathological world, and it strikes hard at the root of the science and practice of sanitation.' Against the above theory, it is contended :—

Firstly : 'That we should judge the unknown by the known. This supposed specific disease virus is not known to chemistry, and there is no known pure chemical agent that acts in any way analogous to the specific virus of fevers, one dose of which, when taken into the stomach, gets into the circulating blood, multiplies itself there a million fold, and produces a constitutional disturbance lasting for weeks or months—as in surra ; and a few drops of that living blood, after the lapse of many weeks since it was first chemically impregnated, if placed under the skin of a healthy animal will, by chemistry alone, multiply and reproduce the same disease again, and so on *ad infinitum*. In considering this subject, we must bear in mind that the same blood does not remain long circulating in the body, but that it is in a constant state of renewal, and, so far as we have any knowledge of pure chemical non-corrosive poisons, when only one moderate dose is given, it either precipitates and remains harmless, because there is too little of it, or else it is decomposed and passed out with the effete blood, and leaves the system uninjured, or else it rapidly causes death. A small dose of pure chemical poison has never been known to multiply itself in the system, nor has there been anything analagous to that performed by any ingeniously contrived apparatus at any chemical laboratory that I am aware of.'

Secondly : 'It is contended that specific self-propagation, like by like, is the most characteristic feature of living organisms, as distinguished from non-vital chemical agents : so that if we never found any foreign living organisms in the blood of specific disease, reason by analogy from known to unknown would lead us to believe that the disease was due to propagating living organisms present in the blood, though unseen.'

Thirdly : 'It is contended that the microscope has enabled us to see such living organisms as an efficient cause, in the blood, to develop the diseases in question. In reply to this, they say that organisms, so like as not to be clearly distinguished by the microscope, are found also in some healthy subjects, and appear to do no harm. Therefore, we should not conclude that the one is any more virulent than the other. The answer to that objection is this, we must not always judge by appearance, but rather by actions. There is an old saying, worthy of being remembered, in our intercourse with men that "A fool is considered wise until he speaks." It is not by vision, but by noticing results, that we usually first learn to distinguish the poisonous snake from another, not very unlike it, which is harmless, and a berry is deadly poisonous, while another, very like it, is wholesome. After we learn that difference in effect, we notice specific differences in appearance, but if these snakes and berries were so small as some of the organisms we find in the blood, no microscope could enable us to see the slightest differences between the poisonous and non-poisonous. We must, therefore, judge them by their results or associations, and we do it in this manner. We often find organisms A, B and C in the blood of healthy animals, and therefore conclude that they are harmless, but we also find organisms indistinguishable in shape from A, B and C present in the first stage and throughout the course of diseases 1, 2 and 3; A always abundant in 1, never in 2 or 3; B always abundant in 2, never in 1 nor in 3; C always abundant in 3, never in 1 nor in 2. The blood of 1, 2 and 3 contain the organisms A, B and C respectively, if put into the stomach or under the skin of healthy animals will invariably reproduce 1 + A, 2 + B, and 3 + C, but not other associations. Again, if we destroy A, B, C, or remove them from the blood of 1, 2, 3, by filtration, and then inoculate healthy animals with the blood so treated, we shall either reproduce the diseases in a very mild form or not at all. A pure chemical virus would be in a state of solution, and would not be affected by the filter, from which results we conclude that the organisms A, B and C are the causes of the diseases 1, 2 and 3, though from other observations we find that organisms indistinguishable from A, B and C in *appearance* are harmless.'

'The parasitic organisms A, B and C may cause disease by secreting chemical virus, or by causing some kind of fermentation in the blood.'

That suffices to give you some idea of the professional controversy I was engaged in at the early dawn of the present day of pathogenesis. Remember how impossible it is to differentiate positively some bacteria in their living state, which are easily differentiated when they are dead and stained. We had no stain for them in India at the time of my report. I may be allowed to state now, after much experience in staining, that we formerly, by close, continued observation of living bacteria, especially the bacilli, noticed differences that few modern bacteriologists are aware of—time is valuable, there is too much haste to fix the bacteria on the glass by passing it through the flame, which alters their shape. Moreover, the pathological changes in the blood corpuscles, in the development of small plates, are too often not observed so closely as in pre-staining days, because of the time necessary to do so.

The Surgeon-General and the Chief Sanitary Officer, and all the senior Medical Officers in India at that time, continued to maintain their opposition to the theory of pathogenesis advocated by me: they officially sat upon me heavily, but the Government printed and circulated my reports, and I have been gratified by the assurance of some younger men that my statements spurred them to follow with much success the line I had indicated for further investigation.

I copy the following extract from my report upon camels I found affected with surra at Dera Ismael Khan:—

'On examining the mucous membranes I found the first four on the list presented the characteristic appearance of surra in horses—yellow with petechiae—but No. 8/79 had petechiae without the yellowness; he was blanched, his eyes were dry, the others wept. Neither of them had dropsical swellings, nor had they any previously, I was told. On examining the blood by the microscope I found the parasite of surra in countless numbers, and very active in the first four, but not one in No. 8/79. I examined many drops of his blood, and failed to find one. He had in his blood what I did not find in the others, the embryos of a filaria which appeared to me to answer exactly the description given by Dr. Timothy Lewis in his monograph on the *Filaria sanguinis hominis*. About ten or twelve were present in every small droplet of blood. I have submitted specimens to Dr. Lewis, but he has not yet had time to determine whether they are exactly the same as those found in man or not, he is inclined to think they are different. The urine of each of these camels was normal.'

Camel No. 8/79 was killed by the usual Moslem method of throat-cutting, for me to make a post-mortem examination, which I reported as follows:—

'There was no abnormal amount of serum in the peritoneal cavity, nor fluff of lymph such as I found in surra horses. The organs were all healthy. But in the right ventricle of the heart, and in the pulmonary arteries, I found tangled masses of adult filaria several inches long, which I supposed to be the mature form of the embryos which I had found by the microscope in the living animal. I have not had time to examine them in detail, neither has Dr. Lewis, to whom I have given specimens, but they are certainly thicker than the single specimen of *Filaria sanguinis hominis* which he found in man.'

Dr. Lewis read a paper upon those I found, giving full detailed description, showing how they differed from any found previously, and named them *Filaria evansi* (*vide Proceedings, Asiatic Society of Bengal, March,* 1882).

Before I was able to complete my official report of my work at Dera Ismael Khan, I received orders to proceed as soon as I could to a very different climate, from extremely dry to extremely moist, to investigate and report upon a fatal disease in ponies on the tea estates of Kachar, and other parts of Assam, which I found to be anthracoid bacilli in the blood, with leucocytosis, etc.

On my return to Calcutta, I found Dr. Lewis had come then with the Government of India—from Simla—and had brought the surra puppy in a wretchedly wasted condition. The parasite swarmed actively in the blood. I was disappointed that Dr. Lewis had not made any further observation upon them, nor made any experiment by transference. He was positive, as he was at Simla, that the parasite was not more pathogenic in the dog than those he found like them in the rats were. He believed the pup suffered only from the common 'distemper' of dogs. I asked him how he knew the rats were healthy: he had not taken their temperature, nor kept them long under observation. A casual observer would not think the pup was not healthy when I gave it to him; he did not think the pup was ill then, but I knew he was because of my closer observation and better knowledge of dogs. Probably an expert in rat pathology would say the rats in question were not healthy. It was useless talking. He and Surgeon-General Cunningham remained obdurate; they seemed to regard me as one with 'a bee in his bonnet.' As I said in the foregoing, I was particularly wishful to utilise the pup to inoculate another bitch, to prove under more careful conditions whether the surra parasite did pass via the lactial glands to sucking pups—that question appeared to me of great scientific importance, whether the microbe was pathogenic or not. I was then transferred from Bengal to be Inspecting Veterinary Surgeon of the Madras Presidency, an appointment for five years, stationed at the Army Headquarters, and ordered to go forthwith. I did not take the pup with me, because I could not keep him sufficiently isolated not to spread the infection—the disease had now been reported in the Madras Presidency—so Lewis destroyed him with poison.

I returned to England, December, 1885, and was stationed at Woolwich as Inspecting Veterinary Surgeon of the district (Army), and, as soon as I could, arranged to study bacteriology in all its practical technicalities at Crookshank's laboratory, King's College, London*—it was delightful work. I expressed to him my wish to experiment in transferring *Trypanosoma lewisi* (*Trichomonas lewisi* he called it) from rat to monkey, to find whether it would become diseased in consequence, and if so, how affected—would the microbe of the rat live and multiply in the monkey? If so, it would probably do likewise in man. The subject was submitted to Lister, to the Presidents of the R.C.P. and the R.C.S., and others, who agreed.†

I retired from the Army in 1890, and settled down here for the convenience of my children's education at school and college. During the University College, North Wales, Session 1890-1, the Council requested me to deliver a course of lectures upon Veterinary Hygiene in the Agricultural Department. I consented, and was re-appointed annually for over twenty years. I resigned on account of my health.

All my medical studies have been *con amore* only, my ample reward being in the scientific attainment. I never applied for any appointment excepting to the Army. I did not try to qualify for medical registration in England. I became a member of the British Medical Association in 1870 on my return from Canada, or in 1871, I forget which.

Now I must conclude, having written much more than I intended in commencing this letter. [I cannot write so fast now as I used to, having cataract in both eyes, so this has been running, or rather walking, for many days.] However, it may not be unfitting if I add to the story of my medical course (what you ask of me) a few lines showing my collateral studies.

I had read the *Vestiges of Creation* before Darwin published his

* I also attended a course of lectures at the Royal Sanitary Institution, and studied practical sanitation there.

† The experiment was not carried out.—EDD.

Origin of Species, which captivated me. Spencer's *First Principles*, published two or three years later, made me sit at his feet ever after. Next came Lyell's *Geological Evidence of the Antiquity of Man*, plus his *Principles of Geology, or the Modern Changes of the Earth and its Inhabitants*—enlarged edition of 1876, and Tylor's *Primitive Culture*, plus Frazer's *Golden Bough*. In biology I was particularly interested in heredity, and the subject of the continuity of Germ Plasm and psychology was the principal object of my thoughts for a long while, reading all the leading investigations I could find published in English to help me.

All such spiritual pabulum I devoured in a hungry state of mind, so I was lifted and carried on the crest of the greatest scientific wave in history. Moreover, I was aged 13, the dawn of manhood, in the great revolutionary year 1848 that shook Europe. The conversations of my father then fixed me in rationalism and democracy. I followed Cobden and John Bright closely. I am sure there would have been no trouble in Ireland *re* Home Rule, etc., if Parliament had settled the land question as Mr. Bright recommended persistently. My father was the first in Western Wales to sign the teetotal pledge, and he formed the first Teetotal Society at Towyn in 1837, and kept his pledge so long as he lived. I was then aged 2, and I have remained teetotal. I have signed the pledge many times, because I found at many meetings in different parts of the world it was a great help if somebody went forward to sign directly after the chief speech was delivered. For some time after I entered the Army I was the only teetotaler in the officers' mess. When I retired from the Army teetotalers were common. My recollection of the changes in the medical use of alcohol is surprising. The subject of the changes in the fashion of medical treatment is interesting; it is curious how many medical men follow the herd, like sheep. The 'psychology of the crowd,'—I must stop at that. If I have bothered you with too much, pardon me.

Yours truly,

GRIFF. EVANS.

25 *October*, 1916.